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## True Parthenogenesis in the Honey Bee.

BY PROF. C. T. E. VON SIEBOLD.

Whilst I was occupied with my task of establishing a parthenogenesis in *Psyche Helix* and *Selenobia triquetrella and lichenella*, I did not omit to bring within the limit of my investigations other insects also, of which the story went, that the females were capable of independent reproduction in the virgin state without the assistance of the male individual. It was of importance to look carefully at the honey-bee, upon the reproduction of which the most extraordinary statements have been made at all times by the various bee-keepers. Amongst these statements my attention had already been turned to that remarkable faculty which was ascribed to certain worker-bees, and which was said to consist in their being able to lay eggs capable of development without copulation.* In the year 1851, therefore, I put myself in communication from Breslau with various bee-keepers, and in this way became acquainted with the distinguished apiarian Dzierzon, pastor at Carlsmarkt, near Brieg, in Silesia. By this apiarian, who is gifted with an admirably acute power of observation and free from prejudices, I was furnished partly in letters and partly by word of mouth, with information upon the economy of bees and the most important phenomena of bee-life, of a kind such as I could never have obtained from zoological and entomological works. What surprised me most in these communications, was the entirely new theory of reproduction which Dzierzon had established, with which he then made me acquainted, and by which all the phenomena relating to the process of reproduction in the bees, which so often border upon the marvellous, may be completely explained.

One of these remarkable phenomena is the property just referred to, possessed by some worker-bees of laying eggs capable of development, a property which is denied by no observant bee-keeper, but could not hitherto be satisfactorily explained in any way. The dissection of the worker-bees had shown that they possess undeveloped ovaries, that the seminal receptacle is only imperfectly developed in them, and that, by reason of the abortion of their copulative organs, they are by no means in a condition to copulate with a drone (a male bee) and allow themselves to be fertilized by him. But whence then should this reproductive faculty of certain worker-bees arise? At first I attempted to bring this reproductive power into connection with the Alternation of Generations, and expressed the supposition that similar circumstances might occur among the bees as amongst the aphides; and that consequently amongst the bees individuals were produced at certain times, which, as nurse-like creatures, could produce brood, without fertilization. But if nurses really did occur in the bee-colonies, these must have been recognizable by dissection, as instead of ovaries they would contain germ-stocks, and no trace of a seminal receptacle. I, at the same time, expressed the wish that I might soon have an opportunity given to me of submitting bees which had been ascertained to be fertile workers, to a careful dissection and microscopic examination, in order to decide whether or no they really were nurses.

But when I became acquainted with Dzierzon's theory of the propagation of the bees, and constantly grew more and more convinced of its correctness, it was evident to me that we cannot speak of a nurse-formation amongst the bees. To inform myself as completely as possible about this theory, I went myself to Carlsmarkt and held a conference with Dzierzon on the 26th of July, 1851, in which I opposed all possible doubts to his theory of reproduction; but these were constantly set aside by him, and

*Hunter in his paper "On Bees," Phil. Trans., 1792, refers to this opinion, but had been unable to confirm it. "It is asserted by Riem that when a hive is deprived of its queen, laborers lay eggs;" * * * and Wilhelm says that it is the laborers only that lay drone-eggs. Hunter then quotes from Schirach: "A young queen lately hatched was put in a hive, which had been previously ascertained to contain no drones, and whose queen was removed; and yet the young bees laid eggs." Upon which he remarks: "There is no mystery in this; but did they hatch?" The definite reply to this question, and the nature of the product of the virgin egg, are amongst the valuable facts established by modern research and observation.

with such convincing reasons which could be brought in accordance both with the anatomical relations of the bees and with the physiological laws of insect and animal life in general, that at last I could no longer hesitate in admitting the correctness of Dzierzon's theory of reproduction.

Dzierzon expressed his views on the theory of the reproduction of bees in the year 1845, in the *Bienenzeitung* of Eichstadt, but without particularly emphasising the most important details of his theory. I consider it necessary to reproduce the views expressed by Dzierzon in that journal, word for word. They run as follows:

"Presupposing what will be referred to and proved in the following numbers, that the queen (female bee) to become good for anything must be fertilized by a drone, (male bee) and that the copulation takes place in the air, I express the conviction, from which all phenomena and mysteries may be perfectly explained, that the drone-eggs do not require fecundation; but that the co-operation of the drone is absolutely necessary when worker-bees are to be produced. Whilst in the higher animals the male is the perfect and ruling creature—the bull keeps together and as it were rules the herd of cattle, and the cock does the same with the hens—the reverse of this takes place with the insects. In the wasps, hornets, humble bees, ants, and especially in the bees, the perfect female forms the central point and holds the swarm together. As even the drones are subordinated to her, they are also in themselves altogether imperfect creatures, for the production of which so many forces and conditions are not necessary even on the part of nature as for the production of the queen, and what is the same thing of the workers. (The ancients even appear to have indicated this by the denomination *fucus*.) The truth of this assertion appears at once from the fact that as everything that is capable of the more difficult and greater effect may also produce the easier and smaller one; so every stock, which is in a condition to produce worker-bees, may also produce drones, when suitable cells are not wanting in the nest; but not inversely. In copulation the ovaries are not fecundated, but the seminal receptacle, that little vesicle which in the young queen is filled with a watery moisture, is saturated with semen, after which it is more clearly distinguishable from its white color. The activity of the ovary in the normal state only commences after copulation, but is not necessarily caused thereby; hence many unfecundated queens lay no eggs at all, whilst others lay drone eggs; and even workers do the latter, although, from their want of a seminal receptacle, I regard them as quite incapable of copulation. I am convinced that such eggs are sufficient for the production of drones, whilst the egg from which a queen or a worker is to be developed must come in contact with the filled seminal receptacle. This is certainly only a hypothesis, and will probably remain so, but one to which every close observer will no more be able to refuse assent, than the hypothesis of Copernicus, that the earth turns round upon its axis, for all the mysterious phenomena in the commonwealth of the bees are very simply explained by it."

In a separate bee-book, Dzierzon subsequently summed up his views upon the reproduction of bees, as a regular theory, in the following manner:

"Therefore, and this must be well borne in mind, in the copulation of the queen, the ovary is not impregnated, but this vesicle or seminal receptacle is penetrated or filled by the male semen. By this, much, nay all of what was enigmatical is solved—especially how the queen can lay fertile eggs in the early spring, when there are no males in the hive. The supply of semen received during copulation is sufficient for her whole life. The copulation takes place once for all. The queen then never flies out again, except when the whole colony removes. When she has begun to lay, we may, without scruple, cut off her wings; she will still remain fertile until her death. But in her youth every queen must have flown out at least once, because the fertilization only takes place in the air; therefore no queen which has been lame in her wings from her birth, can ever be perfectly fertile; I say perfectly fertile, or capable of producing both sexes. For to lay drone eggs, according to my experience, requires no fecundation at all. This is exactly the new and peculiar point in my theory, which I at first only ventured to put forward as a hypothesis, but which has since been completely confirmed. Three young queens with imperfect wings have occurred during the past summer, and these, from the imperfection of their wings, could evidently never have taken the fertilizing flight, and also on dissection proved to be unfecundated, nevertheless laid drone eggs." "By this, all the mysteries which we have hitherto vainly endeavored to unriddle, are completely solved. In the first place the enigma: Why is it that many mothers—they may be either queens or workers in their form—are only capable of propagating the male sex or drones? Because the former are either unfecundated or their fertility is exhausted; the latter, on the other hand, are incapable of fertilization."

"For I am firmly convinced that the egg-laying worker-bees, which occur abnormally, are from the want of a seminal receptacle, just as little capable of being fertilized as the young queen in want of sound wings. Moreover, there is certainly no doubt that by the peculiar tone of her wings the queen allures the drones to her, and disposes them to copulation, of which a worker is of course incapable. In the second place, the before-mentioned power of the fertile queen to lay worker and drone eggs at pleasure, is rendered very easy of explanation by the fact that the drone eggs require no fecundation, but bring the germ of life with them out of the ovary; whilst otherwise it would be inexplicable and incredible. Thus, as it has already been shown that the ovaries are not impregnated, but that the seminal receptacle is filled during copulation, the queen has it in her power to deposite an egg just as it comes from the ovary and as the unfecundated mothers lay it; or by the action of the seminal receptacle, past which it must glide, to invest it with a

higher degree, a higher potency of fertility, and awaken in it the germ of a more perfect being, namely, a queen or a worker bee. This of course she does instinctively, induced by the width of the cell to be furnished." "For the production of the females in the bee-hive, therefore, more conditions and forces are necessary than for the production of males or drones. Every mother which is capable of producing worker-brood, can also lay drone-eggs, but not inversely."

As was to be expected, these views upon the reproduction of bees called forth the most lively contradictions amongst the bee-keepers. They were attacked with the most violent polemics in the BEE JOURNAL above-mentioned, at the same time, however, that most of the opponents being destitute of any knowledge of the anatomical structure of bees, and of any insight into the physiological import of the sexual functions in insects, laid themselves open so miserably that it must have been an easy matter for Dzierzon to silence them. But as it was almost entirely dilettanti speaking to dilettanti, the dispute never came to an end, the most incorrect, extraordinary, and absurd assertions upon the copulation, fecundation, and oviposition of the bees, &c., being put forward in sober earnest as established truths, without its being observed how completely such views, devised in the fancy of a bee-keeper, were destitute of anything like scientific proof. Hence it was possible that simultaneously with the theory set up by Dzierzon, which its originator sought to support by important new evidence from time to time in the *Bienenzeitung*, questions for investigation and reply were again and again propounded in that journal, upon which we must have long been perfectly clear, since the most important points in the reproduction of the bees had been elucidated by Dzierzon's theory.

Thus in the different years of the *Bienenzeitung* up to the most recent time, (1856,) we find the following questions put forward as not satisfactorily answered, and the following points referred to as doubtful by various bee-keepers: namely, whether the drones are really the male bees; whether the drones might not have the care of the hatching of the eggs; whether the drones are not truly abortions; whether there are not also male worker-bees; whether the queen is not perhaps fertilized by caressing or by mere agitation; whether the copulation between the queen and a drone does not after all take place in the bee-hive, and more of the same kind.

In opposition to these variously contradictory questions, I, as Vice President of the third meeting of German Bee Keepers, held on the second of June, 1852, at Brieg, in Silesia, gave an exposition of the anatomical relations of the three kinds of bees, the drones, the queens, and the workers, and called upon the bee-keepers present to express their objections and doubts against the particular points of the theory established by Dzierzon. This was done on several sides; Dzierzon, who was present as president of the society, defended his assertions with the means which his abundant observations, conceived with a correct understanding, furnished to his hand; whilst I came to his assistance with my observations made with the dissecting needle and the microscope, whenever reference was made to the different anatomical relations and the signification of the internal and external sexual organs of the bees.

Although the majority of the apiarians did not so quickly drop their preconceived notions and incorrect views as to the economy, and especially the reproduction of the bees, yet a constantly increasing number of voices was gradually raised in the *Bienenzeitung*, to confirm the correctness of the individual points in the theory of reproduction put forward by Dzierzon. People began to interest themselves in the anatomical structure of the bees and of insects in general; they took notice of the knowledge obtained in recent times by the microscope, by which a clearer view of the function of the male seminal fluid in the interior of female insects had been gained. To strip everything doubtful from those assertions in Dzierzon's theory which still had too much of the garb of a hypothesis about them, and allow them to appear as naked truths, those apiarians, whose sole object was to get at the truth, took care that various individual bees, the exact examination of whose condition might furnish the right explanation of different doubtful points in Dzierzon's theory, were handed over to practiced entomotomists for dissection and opinion. In this way this theory constantly gained in firmness and form, and became strengthened in such a manner that it may now claim to have taken root in the soil of science, there to await a further development. Great merit in regard to the recognition of Dzierzon's theory is due to Baron Berlepsch, of Seebach, near Langensalza, in Thuringia, as that intelligent and experienced apiarian neither shunned sacrifices, time or trouble to obtain the most important information upon the hotly contested questions relating to the reproduction of bees from his numerous bee-colonies, which are extremely well arranged for observation. In a series of apistical letters,* Berlepsch gave a systematic exposition of the new theory of the reproduction of the bees, and supplied the individual positions with proof supported upon the most arduous experiments, by which he has shown himself to be a distinguished observer and acute naturalist.

It must also be mentioned that Dzierzon deserves to be celebrated as making an era not only in the theory, but also in the practice of bee-keeping. He has, namely, given the bee-hive an arrangement, by which it becomes possible for the bee-keeper not only to follow the observation of the individual bee-colonies, and to check the proceedings of their individual members, or of the foreign intruders of the most exact and certain manner; but also to control and guide the entire economy of the individual hives from all sides. He hit upon the happy idea of causing the bees to build their combs

*A translation of the substantial portions of these apistical letters was published in the first volume of the AMERICAN BEE JOURNAL, under the caption of "The Dzierzon Theory."

from transverse slats placed loosely behind one another in the upper space of the bee-hive, by which he was enabled as often as he pleased to examine the whole of the combs in a hive one after the other, the interior of the hive being rendered accessible by taking away a movable back or front wall—as by this arrangement each individual comb, clinging from beneath to the loose transverse slat, can be taken out with this, examined on both sides, and again suspended in its place without injury. By the help of this ingenious arrangement, it had become possible not merely to trace what went on in a bee-hive from day to day, or from hour to hour, he could even convince himself most exactly with his own eyes at any time, as to what was taking place in every individual cell in the different combs in his hives. He was also enabled in this way to procure a knowledge of all the proceedings of the workers in the interior of the hive between the combs, and also to witness the doings of the queen bee. These were all advantages which even the celebrated hives with glass walls could not in the least present, as these latter bee-hives only permitted the surface of a single comb, which was turned towards the glass to be inspected, but otherwise allowed only a very small and extremely imperfect insight into the interior of a colony of bees.

Dzierzon could give the most exact account of the condition of his bee-hives. He knew the number and the kind of cells which were daily or hourly supplied with eggs by the queen; he knew in what time the larvæ in the eggs arrived at their exclusion; he was enabled to watch the gradual growth of the larvæ; he could exactly ascertain what kind of feed was furnished to this or that larva by the workers; he could acquire the most positive information as to the time of preparation of a bee-larva, as to the period of the escape of the bee from the covered cell, and as to the number and nature of the queen cells; in this way he was always informed in what condition the queen governing a bee-hive was; he could detect every disturbance, every irregularity, which induced by multifarious circumstances, easily occurs in the well-ordered economy of a hive, as quickly as its cause.

What advantages this must have afforded to an apiarian endowed with such an acute and unprejudiced power of observation as Dzierzon, may easily be imagined. By this agency the most important and instructive information regarding the proceedings of a bee colony might flow in upon that acute observer, and it could not but happen that at last the extremely remarkable and concealed process of the reproduction of the bees would be correctly penetrated by the eye of man. But even for practice the Dzierzon bee-hives were of the greatest importance, for Dzierzon could know exactly, at any time, and of any one of his bee-hives, how strongly it was peopled, how industrious its inhabitants were, and what they were occupied with. He could always inform himself whether the number of workers was in proportion to that of the brood produced by the queen, whether the number or presence of drone larvæ was or was not useful to the hive, whether the necessary store of food was present, &c. With all this the intelligent bee-keeper and possessor of Dzierzon hives, by the aid of which a complete insight into the state of each household of bees might be attained, could exercise a correcting and directing action by adding the wanting number of necessary workers to a hive which was poor in workers, or taking away some of the combs filled with eggs and brood from scantily peopled hive, so as to lighten its work, and hanging them in an abundantly peopled hive for further care. The careful bee-keeper now knew from what hives he had to remove the combs filled with drone-larvæ which were either unnecessary or dangerous; he was enabled to save a hive, the inhabitants of which, although otherwise industrious, threatened to become demoralized by the loss of their queen from this dangerous state of anarchy, by taking care to replace this loss where the bees themselves omitted to do so. In short with the assistance of Dzierzon's hives, an experienced and careful bee-keeper may go to work like an intelligent gardener, who, by cutting away the unnecessary roots, and attending properly to the bud-bearing twigs, prepares and supports his trees for the production of a rich harvest of fruit.

In turning to the more exact exposition of Dzierzon's theory of the reproduction of bees, I give prominence to the most important points in the history of their propagation, upon the establishment of which Dzierzon must have laid particular stress, as a number of the proceedings in a bee-hive relating to reproduction can only find their correct explanation and elucidation if we maintain *that the young unfecundated queen never copulates in the bee-hive, but always outside of this, high up in the air.* I pass over the lively dispute which has been carried on amongst bee-keepers from time immemorial for the defence or rejection of this point, and only refer to the fact a queen has never been surprised in the act of copulation within the bee-hive by any apiarian who has obtained an insight into the interior of a hive by the employment of Dzierzon's hives. The drones as long as they remain in the hive are always extremely sluggish insects, which are not even roused from their quietude and phlegm by the proximity of a queen desirous of copulation; on the other hand, when a warm, clear, and still day has allured them out into the open air, the sexual and copulative impulse is awakened in the highest degree in these otherwise sluggish drones. They rove through the genial air high over their hives with a loud humming to attract the attention of a queen, who would be impelled to take her wedding-flight by the same favorable weather. At any rate, very few of the many thousand drones attain the longed-for happiness of being selected and accepted by a queen for a husband, it being well-known that the number of female bees is very small in proportion to the great number of male individuals. But by means of this disproportion, the few female bees on taking their wedding-flight, are always sure of attaining their object, as from the number of drones roving through the

air with the same intent, it will not be difficult for a queen to make choice of an agreeable consort.

That the copulation of the bees takes place in the open air, is certainly nothing remarkable, as we see so many other insects perform the act of copulation while flying freely about in the air. It is true that the copulative act is very quickly completed by the bees, and this is proper to all those insects in general, which, with the bees, belong to the order hymenoptera; whilst the males and females of insects of other orders usually remain for days closely united in copulation. For this reason it is one of the rarest events, for even the most observant entomologist to surprise a pair of hymenoptera *in flagranti*. The bee-keepers therefore must not be surprised that the act of copulation in bees has hitherto been so little observed. However, it has accidentally been seen now and then by human eyes, when a pair of bees, united in the act of copulation, dashed down upon the earth from the upper regions of the air. Such isolated observations, information upon which has also been given in the *Bienenzeitung*, are certainly sufficient evidence that the bees copulate outside of the hive.

A still more convincing proof of the occurrence of this act of copulation in the open air is furnished by the appearance and behavior of the fertilized queen on her return from her wedding-flight. The completion of the coitus of such a queen may betray itself even externally; not only does the external orifice of the sexual apparatus, which was kept closed before the wedding-flight stand open, but the torn male copulative organ remains inhering in the vagina, and partly protrudes from it. In order to determine with certainty from its nature what this foreign body, which had often been detected in the vagina of a queen on her return from the wedding-flight really was, Baron von Berlepsch forwarded to me for careful examination on the 21st of July, 1853, one of these queens, from the gaping sexual orifice of which definitely formed parts protruded. The results with which the exact anatomical and microscopical examination of this queen has furnished me, have been given by me in the *Bienenzeitung*, Nov. 26, 1854. By this analysis, I was able to establish, that those definitely formed parts in the vagina of the queen were nothing but the torn copulative organs of a male bee, (drone.) An intimate union of the two sexes of bees must, therefore, have taken place. The remaining behind of torn portions of the male sexual organs in the interior of the female vagina, is, however, a circumstance which occurs not unfrequently in other insects, especially in beetles. With this condition of the external organs of the queen examined by me, the state of the internal generative organs also agreed exactly, for the seminal receptacle (seminal vesicle,) which is empty in all virgin female insects, was in this queen filled to overflowing with spermatozoids (seminal filaments.) This queen, therefore, had returned to her hive certainly fertilized, and would have possessed the power for a long time of effecting the necessary fertilization of the eggs during oviposition with this supply of male semen.

As in the act of copulation of the bees, the penis of a drone is completely protruded outwards, and as no particular muscular apparatus exists for the extension of the penis, the circumstance that the drone only copulates in flight, has an important signification, to which Prof. Leuckart has already called attention. During the movement of the wings, the different air-sacs of the tracheal system of the drone are filled with air, by which means these can act by pressure in the interior of the body of the bee upon the neighboring penis which is to be protruded.

After this single fecundation, a queen-bee can, for a long time, lay male or female eggs *at will*, for by the filling of her seminal receptacle with male semen, she has acquired the power of producing female eggs, whilst before copulation and with an empty seminal capsule, and therefore in the virgin state, she can only lay male eggs.

The second and most important point of the new theory of the reproduction of the bees, is the proposition established by Dzierzon, that "*all eggs which come to maturity in the two ovaries of a queen bee are only of one and the same kind, which, when they are laid without coming in contact with the male semen, become developed into male bees; but, on the contrary, when they are fertilized by male semen, produce female bees.*"

Dzierzon therefore asserts that every egg laid without fertilization by a queen-bee produces a drone, and that every fertilized egg laid by her produces a worker or a queen, according as the larva excluded therefrom is nourished with worker-food or royal-food.

This proposition of Dzierzon's theory necessarily made the greatest noise when it was first announced, and requires above all to be submitted to the closest examination. Before I undertake this examination, I will only remark that one circumstance speaks *a priori* in favor of this proposition of Dzierzon, namely, that by adopting it every phenomenon, however remarkable, in the sexual existence of the bees may be easily explained. But as a time-honored physiological law is at once abolished by this proposition, namely, *that an egg which is to be developed into a male or a female individual must always be fertilized by the male semen*, the affair seems of sufficient importance to be weighed and examined from all sides with the utmost care. I have taken the trouble upon myself and tested Dzierzon's assertion with all the means at my command, by which I have convinced myself in the following way of its correctness.

In the first place, I may appeal to the fact that it is a general occurrence amongst insects, that the females, even when they have not copulated, deposite their mature eggs without fecundation, it is therefore nothing remarkable that a virgin queen deposites eggs. But we must be astonished that these eggs, although unfecundated, do not remain undeveloped; nay, what is more, that only drones or male bees are produced from such eggs. As to the truth of

this phenomenon, plenty of observations are adduced by the apiarians, of which I bring forward only as the most convincing example, the drone-productiveness of a virgin queen with imperfect wings. Every observant and experienced bee-keeper knows the ill-consequences introduced by the queen-bee which has been excluded with crippled wings, and which has acquired the dominion of a bee-hive. She finds herself prevented from undertaking the wedding-flight, but follows the impulse to oviposition, and supplies worker-cells and drone-cells indiscriminately with unfecundated eggs. These arrive at development, the larvæ excluded from them are provided with nourishment by the workers, they grow up, but all of the same size and the same nature; for they are all drone-larvæ. Those which have grown in worker-cells cannot find room in these, and therefore the workers elevate their narrow cells by subsequent additions so as to obtain room, and in this way produce misshapen combs, or combs with the so-called humped brood (*buckelbrut*).

A very interesting experiment was made by Berlepsch in order to confirm the drone-productiveness of a virgin queen. He contrived the exclusion of queens at the end of September, 1854, and therefore at a time when there were no longer any males; he was lucky enough to keep one of them through the winter, and this produced drone-offspring on the 2d of March, in the following year, furnishing 1,500 cells with humped brood. That this drone-bearing queen had really remained a virgin, was proved by the dissection which Prof. Leuckart executed at the request of Berlepsch.

The true cause of such a drone-productiveness in a bee-hive, however, could only be detected by an apiarian as acute and endowed with such a distinguished power of observation as Dzierzon, whilst up to this time the unfortunate occurrence of an excess of drone-brood in a bee-hive has been quite differently and falsely understood by other bee-keepers. They laid no stress upon the fact that such a hive only contained drone-brood, but they merely wondered that such a hive, governed by a queen with crippled wings contained any brood at all; and they endeavored to explain this phenomenon by the supposition that this unexpected brood could only be produced by a fertilized queen. But as the queen from which this brood was derived had been found to be crippled in the wings, they erred in respect to the affair of copulation, and supposed that this crippled brood-bearing queen was certainly fertilized, and that consequently the act of copulation was effected by the queen-bee within the hive. This erroneous conclusion of course brought in a number of other errors with regard to the signification of particular bee individuals, and their functions, by which a correct insight into the process of reproduction in the bees must always have been disturbed. Dzierzon alone did not allow himself to be diverted from the right path in his observations; he maintained that the female bee can only return *fertilized* to her hive after the performance of her wedding flight. He did not, however, content himself with the matter of experience. He went further in his rational way of investigating bee-life. He examined more closely the egg-laying and drone-bearing queens, which, according to his observations, were to be regarded as virgins. He tore away the apex of their abdomen, by which means he succeeded in getting sight of the seminal receptacle, which, in a female bee, is of the size of a pin's head. Dzierzon knew from experience that a fertilized bee in the normal state contains a *milk-white* seminal capsule, which, when crushed, gives issue to the milky seminal fluid. He knew that the empty seminal capsule of a newly excluded virgin queen is not milk-white, but limpid; and he convinced himself that in those drone-bearing queens with crippled wings the seminal capsule was limpid and empty of semen, and consequently in the same state as the seminal capsule of a virgin queen. I have spoken with Dzierzon upon these observations, and as from my own microscopical examination, I was well acquainted with the state of the sexual organs of virgin and fertilized queens, I was thus in a position to judge quite safely, from the description which Dzierzon gave me of his investigations made without a microscope, that he had acquired perfectly correct notions as to the difference in the condition of the sexual organs of a virgin and a fecundated female bee, and therefore could not well have deceived himself in this respect.

Moreover, I felt myself the less inclined to doubt the correctness of these observations of Dzierzon's just reported, as I could not but remember that, according to my own observations, the females of certain *psychidæ* lay unfertilized eggs which are also developed, but inversely, instead of males produce nothing but females. Dzierzon, however, by other observations, furnished me with evidence in favor of his proposition, that drones alone are always produced from unfertilized bees' eggs when they are developed, and that consequently in order to obtain drone-brood, it is not necessary that the queen bee should fertilize the eggs when laying them. As I have already mentioned, it happens now and then in a bee-hive, especially when it has lost its queen, that individual workers lay eggs. This phenomenon has long been known to every experienced bee-keeper; nay, it had already been observed that only drones are developed from these eggs laid by workers; but it is only from the attentive observer, Dzierzon, that we know why such egg-laying workers are always the parents of drones, or, in other words, why only drones are always developed from these eggs produced by workers, if they attain to development. This phenomenon stands in the closest connection with the drone-productiveness of the virgin queen-bees already mentioned.

[TO BE CONTINUED.]

[For the American Bee Journal]

## Questions Answered.

In reply to J. L. McCune, Vol. 3, page 58:

Question 1st. *There will.*

Question 2d. I do not believe any person ever had bees work freely on red clover.

J. H. THOMAS,
*Breeder of Italian Queens.*
BROOKLIN, CANADA.

[Translated for the Bee Journal.]

## Development of the Italian Workers.

BY THE BARON OF BERLEPSCH.

On the 22d of May last, Mr. Axthelm, residing here, received an Italian queen bee from Prof. Mona, of Pollegio, and I assisted in preparing a nucleus hive for her reception. We concluded to avail ourselves of the opportunity to observe the process of development of the workers in its various stages, from the hatching of the egg to the perfect maturity of the insect as a honey-gathering bee, and to spare neither time nor trouble in ascertaining and noting the facts with all possible minuteness.

The nucleus was formed in this manner. We took a comb of brood from a very populous colony of black bees, confined the queen thereon in a cage, added a comb containing honey, and four empty combs, supplying some of the cells of one of these partially with water. Having these arranged in the nucleus hive, we brushed off into it all the workers from six frames, allotting the few Italians which accompanied the queen on her journey to a distant colony. Consequently, in the experimental nucleus the queen alone was of the Italian race, the workers being native or black bees.

On the 23d of May, at precisely 7 o'clock in the morning, the queen was liberated, and on the 24th, at 7 a. m., we found twenty-one eggs in the cells of one of the empty combs, but not an egg was found in the brood comb, all the cells of which contained uncapped brood. We had purposely selected such a brood comb, in order to be able to ascertain exactly when the queen would begin to lay eggs.

As already stated, twenty-one eggs were found in the cells precisely twenty-four hours after the queen was liberated. These eggs had in all likelihood been laid very shortly before 7 o'clock on the morning of the 24th, as they were *so few* in number; and it may hence be fairly inferred that a fertile queen, suddenly interrupted in ovipositing, would require nearly twenty-four hours after liberation before her ovaries could resume their function. This queen had been prevented from laying nearly five days during her transit from Pollegio to Coburg. Not a single egg was found in the piece of comb in the transport hive, and it was scarcely possible that it should have contained any, as nearly every cell was stored with honey.

We allowed the queen to pass over on another of the empty combs densely covered with bees, and then set this in the sun that we might see her lay. At thirteen minutes past seven she laid the first egg, and after laying five eggs more before a quarter of eight, we replaced her in the hive and removed the comb containing the twenty-one eggs previously laid. We also brushed off the bees from the other empty comb inserted on the 22d, and removed it from the hive, that we might be positively certain that the oldest egg in the nucleus was laid on the 24th, at thirteen minutes after seven o'clock in the morning.

On the 26th of May, at thirteen minutes after seven in the morning, precisely forty-eight hours later, not one of these eggs was yet hatched. To be certain of this, both the combs containing eggs were lifted out and the bees brushed off, that each of us might carefully examine them. Again, at thirteen minutes past twelve at noon, at thirteen minutes past three in the afternoon, and at thirteen minutes past seven in the evening, not a larva was yet disclosed. But on the morning of the 27th, at thirteen minutes past five o'clock, sixty-nine eggs were hatched. Thus larvæ were dislosed from sixty-nine eggs in the interval between sixty-one and seventy hours. I do not say in from sixty-one to seventy hours, because at fifteen minutes past seven o'clock on the morning of the 24th only six eggs had been layed.

This corresponded with an experiment I made in 1859, but was in direct contravention of what Gundelach alleges, (in his Supplement, page 23,) that the larva emerges from the egg in twenty-four hours. Hundreds of observations have taught the incorrectness of this statement.

On the 3d of June, at thirteen minutes past five o'clock in the morning, six days accordingly after the hatching of the eggs, not one cell was yet sealed up, though it was evident that arrangements for sealing up were in several instances being made, as the inner margins of some of the cells were obviously broadened. At noon two of the cells were already so nearly sealed over that only a small central hole was still perceptible. At thirteen minutes past five o'clock in the evening nine cells were completely closed. It was thus found that in six and a half days after the hatching of the eggs the first cells were sealed over. This harmonizes essentially with one of my experiments made in 1859, and with those of Gundelach as detailed in his Treatise in 1842 and in his Supplement in 1852, but directly contradicts the statement of Huber, who assigns five days as the term that the larva remains unsealed in the cell.

On the 11th of June, at thirteen minutes past seven in the morning, precisely eighteen days after the hatching of the eggs, none of the brood had yet emerged, though the appearance of the cappings on the comb first supplied with eggs showed plainly that many young bees were nearly mature. At twelve o'clock, noon, and also at three o'clock in the afternoon, no bees had yet left their cells. But when we renewed the examination at thirteen minutes past seven in the evening, we had the great gratification of seeing that two bees had just cut through the caps of their cells and were about to emerge. In a minute later both were *born*.

It was thus ascertained that an Italian worker may be maturely developed *in eighteen days, twelve hours, and about one minute.* At thirteen minutes past eight o'clock in the evening, just before dusk, we re-examined the hive, and found that six bees had left their cells. On the 12th of June, at thirteen minutes past seven in the morning, precisely nineteen days after the first egg was laid, at least two hundred and fifty bees had emerged. It was thus further ascertained that, in the summer season, the Italian

worker does not ordinarily require *nineteen full days* for its perfect development.

From the 12th of June forward the hive was closely observed, daily, between the hours of twelve at noon and three o'clock in the afternoon. On the 16th, on the fifth day accordingly, (counting from June 11th fourteen minutes after 7 o'clock in the evening, when the first two bees were born,) no Italian bee had yet shown itself outside of the hive. As on the 17th and 18th the thermometer fell to 60°, so that, though the locust trees were then in full bloom, very few old bees left their hives, we feared that we should find our experiment partially defeated when warm weather recurred on the 10th. For if Italian workers were seen to issue in numbers on that day, as the eighth, it would not follow, as a matter of course, that they do not leave their hive till on the eighth day after emerging from the cell, because the cold weather experienced on the sixth and seventh days (June 17 and 18) may have prevented them from making their appearance. But to our great joy, our apprehensions proved unfounded, for on the 19th, the eighth day, only a single Italian bee presented herself on the alighting-board, without attempting, however, to leave it, but soon retreated into the hive again, though the thermometer, at the moment of her appearance, (three minutes past 1 o'clock,) stood at 72°.

On the 20th, the ninth day consequently, we saw only four Italian bees leave the hive hovering in its front, and speedily returning. The thermometer stood at 72° in the shade. But on the 21st of June—or on the 10th day—when the thermometer stood at 78°, the Italian bees flew sparingly, and in increased numbers not before the 22d.

This result is quite remarkable, and does not accord with any previous experiments. Thus:

1. Besides the utterly abortive experiment which I made in 1856, I made three new and very precise ones in 1864–6, which uniformly indicated the *eighth* day as that on which the young bees first made their appearance in considerable numbers in front of their hives, though a few showed themselves on the *seventh* day, on the alighting board, but retired again without attempting to fly. Earlier than on the seventh day, I have never seen young bees leave their hives, although, besides the experiments now referred to, I have made observations in forty or fifty instances.

2. Mr. Kalb, who assisted me in the previous experiments, can corroborate the statement that not a bee left the hive prior to the seventh day; and Mr. Hopf, who, in 1864 and 1865, Italianized at least sixty stocks of black bees, informs me that "*before the seventh day no young bee leaves the hive.*"

3. Dr. Dönhoff distinctly mentions the *seventh* day as that on which the young bees first left his experimental hive.

4. Dzierzon says "after leaving the cell, a week passes (*seven* days) before one or more young Italian bees will make their appearance before the hive while the bees are flying." This agrees precisely with the statements under 1 and 3. In his treatise on "Rational Bee Culture," he says more vaguely "*several* days;" and it would seem as if he wished to reduce the period within seven days, thus approximating one his earlier statements, according to which the young bees made their first appearance outside "about the third day."

5. Bottner, in the *Bienenzeitung* for 1864, page 138, says "at least *six* days."

6. Wittenhagen, in the *Bienenzeitung* for 1866, page 48, says: "In about *eight* days the young bees become strong enough to venture to leave their hives on a warm, calm day, to discharge their fæces."

7. The Novice, of Coblentz, (*Bienenzeitung*, 1864, page 188,) says: "I once saw young bees fly out on the *fourth* day;" and Fisher makes a similar statement in the *Bienenzeitung* for 1863, page 31. Decidedly as these statements differ from mine, I will not undertake to discredit them, because the assertion is direct and positive, and both the Novice and Mr. Fisher are accurate observers. In such cases much depends on by whom the observation is made. Many observers are very superficial, drawing hasty conclusions, without noting the facts carefully, or preserving well-ascertained data for future use. Only too frequently are surmises and conjectures substituted for facts, and conclusions deduced from insufficient premises.

8. Schiller, (*Bienenzeitung*, 1861, page 98,) says "the workers fly out the day after they are born, for purification, and thenceforward regularly in quest of pasturage, *which needs no proof.*" No experienced observer would make any such statement. A bee not more than twenty-four hours old, cannot fly at all, but drops to the ground almost perpendicularly, if thrown into the air.

So far as I know no further statement in this connection has appeared in the *Bienenzeitung*, for what Prachel says (B. Z., 1855, page 123,) is too vague to be of use.

I can say very positively that in my last experiment, the young bees first began to fly out on the *tenth* day. How is this to be explained, in view of my own previous observations and those of others? I will hazard these surmises:

1. The nucleus colony was weak, and the hive disproportionately large. Hence the bees were not crowded, nor could a high internal temperature be maintained; and the influence of warmth on the activity of bees is very well known.

2. The queen had laid an unusual number of eggs. Hence, on the 18th and 19th of June, when the young bees should have come forth, the brood-combs were not densely covered; and on the 18th the weather was too cold to allow bees to fly.

On the 23d of June, the twelfth day after the birth of the oldest of the young bees, and thenceforward, Mr. Axthelm and myself watched the hive with the most scrutinizing attention, relieving each other like soldiers on guard. But it was not till the 30th, between 1 and 2 o'clock in the afternoon, consequently on the 19th day, that we first saw young Italians bringing in *pollen*—three then came laden with pellets. The weather was highly favorable from the 23d to the 30th inclusive, and the locust trees were in full bloom, yielding honey in abundance.

Nevertheless we could not find any young Italians bringing in *honey* till the 30th, though we examined about twenty individuals daily during that time. Only on the first of July a considerable number of Italians brought in *pollen* and *honey*. Hence, leaving out of account the three pollen-bearing bees seen in the 30th, the young Italians first began to gather honey and pollen from the surrounding pasturage on the twentieth day after their birth.

This result, also, does not correspond with any previously obtained.

1. In my three previous experiments, the young bees frequented the pasturage on the *sixteenth* day.

2. Mr. Hopf assured me that in his apiary the *sixteenth* day always proved to be the first on which the young bees went honey-gathering.

3. According to Dr. Dönhoff, (*Bienenzeitung*, 1855, page 163,) the bees first gathered honey and pollen on the *nineteenth* day.

4. Bottner saw a few young bees bringing in honey and pollen on the *twelfth* day, but not in numbers till on the *thirteenth* and *fourteenth*.

5. The Novice of Coblentz found a few young bees bringing in pollen on the *twelfth* day, but usually not before the *fourteenth*.

6. Fisher once saw young bees bringing in pollen on the *tenth* day.

7. Count Stosch states (B. Z., 1860, page 278,) "a bee must be at least *two* weeks old before she goes out foraging." On page 285, he names the *sixteenth* day.

8. When Schiller says that it requires no further proof that the bees fly out for purification the day after they are born, and thenceforward regularly in quest of pasturage, it only requires no further proof to show that he is a poor observer.

9. Wittenhagen remarks that "at the average age of *fourteen* days the bees commence regular labor."

10. Dzierzon's casual statements relating to this point, are too vague to be taken into account.

According to the foregoing, I conceive that for the present, and until better advised, we may assume that ordinarily the bee leaves for foraging on the sixteenth day. From this, according to locality, some important practical considerations follow. In most sections of the country the supply of pasturage fails prior to the 10th of August—that is, after that date hives rarely increase in weight. Now, if we assume *nineteen days* as the term required in summer for the perfect development of a worker bee from the egg till it leaves the cell, *thirty-five days* elapse before it becomes a perfectly active honey-gathering insect. Consequently in such a district, all the eggs laid after the sixth of July will produce bees that cannot aid in any productive labor in that year. Hence the truly intelligent rational bee keeper should diligently endeavor to have only as many workers bred after the twenty-fourth of June, as will suffice to keep the hive populous enough for wintering well.

It is important to know at what age precisely bees ordinarily commence active labors. I would, therefore, request the correspondents of the *Bienenzeitung* who desire to advance rational bee-culture, to institute numerous and exact observations next year, so as to elucidate this subject more fully.

Practice, unless based at all points on correct theory—or, if I may so express it, practice that is not truly *applied theory*, is mere bee-keeping relying on *good luck*. Correct practice can only emanate from correct theory; and he who is either too dull or too indolent to study the theory so as to comprehend it clearly and be able to apply it, may *keep* bees, but knows nothing of bee *culture*. The more clearly theory in all the minutest details is evolved and developed, the more definite and precise will the practice become, and the less necessary will it be to propound rules. He who is thoroughly grounded in the theory will always know how best to proceed in practice; whereas he that is wholly or partially unacquainted with the theory will scarcely ever know how to proceed, though he be furnished with a volume of empirical instructions.

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For the American Bee Journal.

## Italian Bees.

Doctors differ quite as much in relation to Italian bees as sick patients. It is true that in most respects all agree that the *pure* Italian bees are superior to the natives, but all depends on their purity; and here is where the "difference comes in" One dealer in Italian bees says "the workers are distinguished from the natives by a yellow band around the abdomen;" another says "three yellow bands or rings;" a third makes the markings of the queen a test of purity; a fourth tests the purity of a queen by her progeny; and a fifth makes the very amiable disposition of the workers, or the "impeccability of temper," a test of purity A person entirely unacquainted with Italian bees, after hearing the different opinions of these doctors, if he purchased a queen purity guaranteed, would hardly know where to look for a reliable test. In fact, will these different parties who are engaged in the sale of queens purity guaranteed, forward another if the progeny of a queen sold does not come fully up to their own standard? For instance, suppose a queen is purchased of a party claiming that the queen progeny of a pure queen should be duplicates of the mother, and the purchaser finds in rearing queens artificially early in the spring or late in the fall, that they differ very much in color, will another queen be forwarded? Or will the purchaser learn for the first time that the queen is not expected to duplicate herself except in the swarming season? Again, suppose a queen is purchased of a party claiming three yellow bands as a test, and her workers do not *all* show three yellow bands, will the purchaser be supplied with another, or will he be told that it is only when the bees are young and the abdomen distended, that the three yellow bands can be seen? And if so, is this the fact? One writer for the AMERICAN BEE JOURNAL claims that they should show three yellow bands under all circumstances, whether old or young, abdomen

distended or contracted. Which is correct? Again, suppose a queen is purchased of a vendor of these one-banded d——ls and her workers do not *all* show plainly even one yellow band, will another queen be sent? Or will the buyer be coolly informed that the Italians are a variety of the common bee, and are liable to "*strike back to the original type;*" that "my bees have *improved* since I purchased them, and must be pure?"

Now, dear JOURNAL, is there not considerable, if not more, humbug about this Italian bee business? Are not purchasers semi-occasionally, if not oftener, victimized in purchasing queens? It so seems and *feels* to me to the tune of the first cost of the queens, the loss of a hundred dollars' worth of surplus honey estimated, and the time and trouble of hybridizing an apiary, to say nothing of the extra stings. As I am a novice in *pure* Italian bee-keeping, I do not propose to enter into the present discussion in relation to these various tests of purity, but hope it will be continued until some standard of purity is established, upon which all parties can agree. It has been seven years, I think, since they were first brought to the United States—quite long enough for somebody to have learned something about Italian bees.

VICTIM.

[For the American Bee Journal.]

## Overstocking.

There are often too many bees kept together. I got more honey from forty old stands last season than I ever got from two hundred; and bees carried away, when I had many at home, did better.

There never will be bees enough to get all the honey, or to rob the air of the fragrance that exhales from opening bloom. But honey secretes faster than it exhales, and the success of bees depends on the amount of surplus. If there are so many bees that they find nine out of ten places already rifled, and the tenth with only a small supply from having been recently visited, much of that little will be needed to repair the wear and tear of collection, as the bees lose the greater part of their time and labor in flying from place to place examining empty blossoms.

If there are so few bees that they find three-fourths of the flowers they visit supplied with a good accumulation of honey, more will be obtained in less time and with less labor, and a smaller proportion will be needed to sustain the laboring bees.

A small apiary will therefore afford more honey in proportion than a large one.

LEMONT, ILL. T. H. MINER.

P. S.—The dearth here is severe. Bees doing poorly. More honey in proportion to combs and bees than usual.

AN Iowa bee-keeper travelling through that State recently, remarks: "A few Italian stocks which I chanced to find, worked busily on the flowers of the great Western prairie. I counted twenty-six varieties of flowers visited only by the Italian bees."

[For the American Bee Journal.]

## Purity of Italian Queens.

It appears from a foreign paper in my possession that Mr. John Lowe, of Edinburgh, with a view to test the Dzierzon theory, set to work to obtain hybrids between *apis mellifica* and *apis ligustica*, and also between *apis mellifica* and *apis fasciata*. The result of his experiments, which I give in his own words, was "that Ligurian queen-bees fertilized by English drones, and Egyptian queen-bees fertilized by English drones, both produced drones, which, as well as the workers, were hybrid in their characters and bore unmistakeable evidence of the influence of the male parent." From this Mr. Lowe drew the conclusion "that the eggs of a queen-bee which has been fertilized by a drone of another race, whether they develope into drones or workers, are in some way affected by the act of fecundation, and that both sexes of the progeny partake of the paternal and maternal character of the parents or race, from which it follows that Dzierzon's is not the true theory of reproduction in the honey-bee."

Now while I fully endorse the conclusion arrived at by Mr. Lowe, "that drones are in some way affected by the act of fecundation," yet I cannot say with him that the Dzierzon theory "is not the true theory of reproduction in the honey-bee." I fail to see that the Dzierzon theory is materially crippled by the fact of the drones being *in some way* affected by the act of fecundation. The pith of the Dzierzon theory is that all the eggs in the ovaries of the queen-bee are unimpregnated; that the eggs which produce workers are impregnated when passing through the oviduct by coming in contact with a sperm reservoir and receiving a minute portion of its contents, while the eggs that produce drones pass the sperm reservoir without coming in contact with it, and hence are not impregnated. This may be true, and still it may be a fact that drone eggs are *in some way* affected by the act of fecundation. But the deduction generally drawn from the Dzierzon theory that drones are therefore *pure*, cannot be strictly true, if a queen has mated with a drone of another race. Neither is Mr. Lowe correct in saying that such drones are hybrid in their character. The truth lies between the two extremes. Mr. Lowe has simply discovered what others had discovered before him, namely, that drones are *in some way* affected by the act of fecundation. He does not attempt to explain that "some way," but jumps at the conclusion that they are hybrid. I will, therefore, for the benefit of the honest breeder of Italian queens, explain how drones are affected by the act of fecundation. The truth is that the whole system of the queen-bee is affected or changed by the act of coition. In other words the life-giving principle received from the drone by the queen into the sperm reservoir is also, by absorption and circulation, carried through the whole system and becomes a part of her very nature, and hence is transmitted to her progeny. It will then at once be seen that if a pure Italian queen cohabits with a black drone,

her eggs, which are a part of herself, will partake to a certain extent of the nature of the drone. Therefore, her drone progeny, although not hybrids, will show unmistakeable evidence of the influence of the male parent. Not only so, but the eggs of a queen are affected by the impregnation that produced herself. In other words, the impregnation of an egg does not end with the production of a queen-bee, but through her system is transmitted to her eggs sufficient to cause them to produce drones. In this way we can without difficulty account for the production of drones from the egg of an unimpregnated queen.

If breeders of Italian queens will accept and acknowledge the above truths, there is an end to the discussion of the purity and non-purity of the "three-banded," "two-banded," and "one-banded" bees. For it must be clearly seen that if queens to the third and fourth generation continue to mate or cohabit with drones produced from a queen that has mated with a common drone, their progenies, though not hybrids, will show the influence of the black race by the number of bands, some of the bees losing one and perhaps two bands. Hybrid bees not only lose *one* and *two*, but even the three bands, appearing as black as the native bees—showing the Italian blood, however, in the shape of the abdomen, which is more pointed than that of the black.

In order, then, to improve the purity (if I may be allowed the expression) of our Italian bees, it is only necessary for breeders of Italian queens to destroy all drones produced by what we call hybrid queens, or queens that have mated with common drones, allowing their queens to cohabit only with drones produced by queens that mated with an Italian drone. This every honest breeder will endeavor to do when purity of race is desired.

But while speaking of the purity of Italian bees, I must say that I fail to see that Professor Varro's "impeccability of temper" and A. J. Root's "breathing" process are of any value in testing the purity of Italian bees, for two reasons. *First*, neither test is needed, or of any avail in proving their purity. *Second*, both are liable to failure. It must be at once conceded that impeccability of temper, or a disposition to bear being breathed upon belongs only to three-banded bees; and as a hybrid queen never produces a full colony of such, therefore three-banded bees are pure, and no further test is needed, Again, there are times when Professor Varro's bees will be found wanting impeccability of temper. If not when operated upon by himself, they will when operated upon by some other person, the effluvium of whose body shall not be so acceptable as that of his own. Nothing can be more certain than that some persons are far more likely to be stung than others. I think my Italians pure, but I find them quite too ready to sting, yet not so much inclined to do so as the natives or the hybrids. If Professor Varro or any other person has Italian bees that will not sting me, let them set their own price and they shall have it.

J. H. THOMAS.

BROOKLIN, ONTARIO.

[For the American Bee Journal.]

## Wintering Bees in the Ground.

MR. EDITOR: In response to the inquiry of W. T. in your September number, page 58, I would say that low and uniform temperature, dryness, darkness, tranquility, security against mice, and slow renewal of air, are conditions required for wintering bees in the ground. I use the mode which experience has proved successful.

In well drained sloping ground, I dig a ditch half a foot deeper than my hives are tall, and one foot wider than they are broad. I drain that ditch for greater security. If fearing the falling in of the earth, I stay the ground with some old planks. Then I lay in the bottom two 4x4 inch beams. Upon these I place my hives, having previously raised them from their bottom boards by inserting strips of half inch laths. I remove top-boxes and leave open all the holes in the honey-boards, in order to give the bees plenty of air. Then with plaisterer's laths I frame pipes or flues, the longer ones descending to within four or five inches of the bottom; the shorter ones to be placed in the roof. I place one of these flues at each end of the ditch, and another after each third hive—alternating a long and a short one. Finally, I prepare a support for a double-sloping roof of old boards; and then cover the roof with straw nearly a foot high, and place on that a layer of earth equally thick—making together eighteen or twenty inches.

By these means bees are maintained in a low temperature, and remain dormant for months, consuming little honey; and are all alive and active in the spring.

This is the best way to preserve feeble and poorly supplied stocks.

Last year I wintered some third swarms in the ground, giving them honey in boxes, which remained untouched—the small quantity of honey they had in their hives, having been sufficient for their support.

I came from France four years ago, and am my own instructor. I shall receive the *French Bee Journal* and will translate for you such articles as I may deem serviceable to bee-keepers in my new country.

CHARLES DADANT.

HAMILTON, ILL.

[For the American Bee Journal.]

MR. EDITOR: Referring you and the readers of the "JOURNAL" to my former article, (see BEE JOURNAL, Vol. 3, No. 4, pages 63 and 64,) I propose, after relating a further experiment in bee-culture, to give the result of such experience during the bee season just now closed.

On April 1, 1867, after careful examination, I found that out of thirteen colonies of the fall of 1866, I had eight in fair condition, two quite weak and light, and four dead ones, or rather the remnants thereof. On April 14th, I discovered that one of my weak colonies was being robbed by one of my strongest. On this I at once removed the colony that was being robbed from its stand, opened and examined it, and

finding a healthy looking and fertile queen, with some brood and stores, I removed the robbing colony to the stand of the robbed one, and placed the robbed colony on the stand of the robbing one. After a few minutes the bees became quiet, the robbing ceased, and both colonies did well from that day forward. This expedient in this instance proved to be the very best that I ever tried or ever saw tried to prevent robbing. Whether it will prove effectual or not in other cases, trial and time alone will determine.

My further experiments during the past spring, are stated in brief in my former article. I may remark here, however, that in each case where I started an artificial colony, using a sealed queen cell as a basis, I used a full comb containing honey, brood, &c., in which I inserted the queen cell. I also remark that during the present season, I started and built up from the nine original colonies which I had on the 1st of April last, eighteen full colonies of bees, with an abundance of stores for wintering, and have taken surplus, which was deposited in top boxes, about 175 pounds.

All increase of colonies after my experiment of April 28th, heretofore alluded to, was effected by using a sealed queen cell and one or more full combs of honey, brood, &c., placed in an empty hive and giving it the place of a full colony which was overstocked with bees. In no instance did I resort to either drumming or the use of smoke.

I invite no controversy, but do invite information as heretofore, for which see my former article. In other words, if any reader of the JOURNAL has succeeded better than I have in the increase of full colonies, amount of surplus stores, and above all, in securing uniform straight combs, I should be pleased to learn how it was done.

BELMONT.

## Crystalization of Honey.

I have several times seen it stated in the *Scientific American* and elsewhere, that the crystalization of honey is caused by the action of light. In opposition to this theory allow me to present two facts. We frequently take up honey late in the season, (in November and December,) place on tin pans and set them on shelves in the cellar. Some of the honey, of course, leaks out of the cells, and in a few weeks will be found crystalized in the bottom of the pans. Yet no light enters the cellar from the time we bank the house in October till some time in March.

Again, our strained honey we put in jars, and after replacing the covers, set them in a dark closet where no light enters. In the spring the honey that remains unsold or unused, will be found completely "candied." In my opinion exposure to the air and cold have more to do with the crystalization of honey than light.—J. L. W. in *Scientific American*.

☞SEND us names of bee-keepers with Post Office address.

[For the American Bee Journal.]

## Several Points Considered.

1. A writer asks on page 58, volume 3, of the BEE JOURNAL, "Has any one not raising queens for sale ever had bees to work freely on red clover?" I am not raising queens; have none for sale, but have repeatedly seen Italian bees working freely on red clover. I consider the insinuation contained in the question unjust. I have a hybrid stock from a black queen that collected light colored honey last August, while pure black stocks were gathering freely from buckwheat a very dark colored honey. I do not know the source of the light honey, but suspect it was from red clover.

2. Another writer on page 50, volume 3, of the BEE JOURNAL, heads an article "A singular case." I think the case a plain one. The colony referred to had a superannuated queen; that is, a queen whose stock of spermatozoa was nearly expended, causing her to lay many unimpregnated or drone eggs. The instinct of the bees led them, under these circumstances, to supersede her with a young queen at the earliest moment in the season, which they did by raising a queen from one of her *few* worker eggs. You will find an account in the last May or June number of the JOURNAL of a similar proceeding as early as February 28; but this was in a warmer latitude, namely, in Kentucky. I am inclined to think from these two observed cases, that aged queens are often thus superseded, and at so early a period in the season that the fertilization of the young queen is difficult and often impossible; and hence the loss of colonies late in the spring that have passed the winter successfully. I think it would be an advantage to allow no queen to remain in an apiary longer than two years. Kill them as soon as the honey harvest is over, or sooner, and let the bees raise new ones while drones are abundant. Of course it would be necessary to watch such hives as are deprived of their queens to prevent robbing, and to be sure that they obtained fertile young queens. A still better way would be to raise the queens in nucleus boxes.

3. Mr. A. J. Root, page 53, volume 3, of the BEE JOURNAL, gives a pretty good result from a colony of Italian bees, and calls for more figures in regard to common bees this season. Here are some. A double swarm of black bees hived June 28 in an empty hive; that is, without any old comb to assist them, filled the lower part of a two-story glass Langstroth hive, holding twelve frames, and deposited about seventy-five pounds of surplus honey in the second story, entirely free from brood and bee-bread, and mostly sealed over. Another double swarm of black bees hived July 1st, gave me thirty-six pounds of honey, besides handsomely filling thirteen frames for their own use in the body of the hive. The hybrid swarm mentioned above, aslo hived July 1st, did just as well as the last mentioned double black swarm, viz: filled thirteen frames and furnished six boxes of surplus honey, weighing thirty-six pounds. Four other black swarms hived the same day,

all filled the body of their hives, thirteen frames, but yielded no surplus. They were small swarms None of them had any old combs given to them, for I had none to give. I would have paid a dollar a sheet for perfect empty combs for their use, and would have made money by the operation. The only swarm I had last year, hybrids, gave me this year twenty dollars worth of surplus honey; and one swarm forced July 24th, has completely filled its thirteen frames, and, I believe, would have produced considerable surplus honey if boxes had been given to it. Yet, notwithstanding such success with black bees, I intend to Italianize my apiary next season.

4. From an inspection of the combs in all my hives, I find a "general rule" of irregularity in comb-building, namely, the five or six central combs are invariably straight and within the frames. Outside of those, on each side, they are inclined to curve towards the middle of the hive, and sometimes cross from one frame to another near the ends of the frames. In the spring of the year, after the combs become hardened with age, and are comparatively empty of honey, it is not a difficult matter to rectify all this by straightening all the combs. To do it, remove the hive from its stand, and set an empty hive, (presuming that all your hives are of the same size, as they ought certainly to be), in its place. Take out all the straight combs from the central part, and set them carefully in their own order in the empty hive with all the adhering bees. We come now, perhaps, to two frames joined together with comb. Cut away the comb carefully from the frame to which it is least attached; remove the frame, brush off the bees into or in front of the hive, being careful in all the operations not to injure the queen (there is in fact no necessity for injuring a single bee); lay the frame on a board or table, and with a knife crowd the comb into the frame just where you want it. This is best done, not by laying the knife on the comb and pushing, but by placing the knife between the comb and frame, and prying it into place gradually. A common table knife is best. If the comb is built too thick in certain places, as is often the case, slice it off to the proper thickness with a *hot* knife, heated by holding it for a moment against a *hot* flat-iron. Draw the knife rapidly through the comb, and it will not bruise a single cell. Heat it again as often as it cools. You will be surprised to see how nicely even a dull knife will cut under the circumstances, if you have never tried it before. I think, however, that a very simple expedient will secure all straight combs. I have never tried it, for I have never had the meens at hand. In fact, it has occurred to me only since seeing the combs built in my hives this summer. I raise the back end of the hives three or four inches higher than the front, while the combs are building. As above stated, several combs situated centrally are invariably straight. The irregularity begins towards each side of the hive. Now my idea is to place in the hive before hiving the swarm, two straight combs, one at each point where the curved combs usually begin, say at one-third of the width of the hive from each side. This would divide the hive into three equal parts or spaces, each so narrow that the bees would hardly have room to deviate from the straight line enough to carry one comb across two frames. Will some one who has spare straight combs try this next year, and report the result?

R. BICKFORD.

SENECA FALLS, N. Y., Oct. 7, 1867.

## Correspondence of the Bee Journal.

## ITALIAN BEES IN THEIR NATIVE HOME.

### LETTER FROM MR. ADAM GRIMM.

BELLINZONA, (CANTON TESSIN,)
September 12, 1867.

On my arrival on the European continent on the night of the 27th of August, I concluded to visit Mr. Dathe, at Eystrup, near Hanover, who is well-known as an expert and successful cultivator of Italian bees. My purpose was first to compare the Italian bees and queens which I had brought with me across the ocean with those of Mr. Dathe; secondly, to visit the heaths of Luneburg that I might personally examine the bee-stations there; and thirdly, to ascertain whether the Italian bees there reared are less disposed to sting than their native black bees—conceiving that I should thus enjoy the best opportunity to form a correct judgment on this point. On arriving at his residence I found Mr. Dathe so much occupied in sending off queens, that he requested me to call again next day, when he expected to be more at leisure. On the following morning he called for me at the hotel, and I accompanied him to his house, where I opened the two nucleus hives I had brought with me, and gave the bees an opportunity to fly. Very few workers had died on the voyage; but I had the mortification to see one of the queens, which I was showing to Mr. Dathe on the comb, suddenly take wing and leave for parts unknown. After minute inspection Mr. Dathe declared that my workers and queens were fully equal to his own. He remarked further that I would not obtain equally handsome queens and workers from Professor Mona; though he by no means intended that the remark should imply that Prof. M.'s bees were not of the pure Italian race. Mr. Dathe then kindly showed me his arrangements for queen-raising and several unfertilized young queens. I found these all of a beautiful yellow color, there not being a dark or brownish one among them. After close comparison I could perceive no difference between his bees and my own. In reply to my question whether all the young queens produced were uniformly of the same color as the mother, he said that such was by no means the case. He, however, had a queen last year, from which he reared a hundred yellow young queens before he obtained a dark one, and that one was nearly black; but that I might calculate on finding nearly one-half of Prof. Mona's queens dark. We then proceeded to his heath apiary, situated at a dis-

tance of not more than 1½ miles. I found there about one hundred colonies, mostly in Dzierzon movable comb hives, though there were among them a few colonies in common straw hives—these having been bought by Mr. Dathe from some of his neighbors. This, he said, became necessary, as he had sold nearly all his old stocks last spring. So long as we were in the vicinity of Italian stocks, we were very little annoyed by the bees; but when we approached the black bees at the heath apiary, I was repeatedly stung, though I was equipped with a bee-cap, and I then removed to a respectful distance. Mr. Dathe said that he uses a bee-cap when working among his bees, and this was especially necessary when his black bees were at work on the blooming heather, as they were then peculiarly ill-tempered. Only too soon did I become convinced of the correctness of this remark. We went to visit another heath apiary only a mile further off, and while we were yet about ten rods distant, the bees attacked us. We nevertheless advanced to count the hives and inspect their interior arrangement. There were 174 stocks placed in a square, facing the cardinal points, set in two tiers with the hives not more than six inches apart. The whole was inclosed with boards, and the hives were of straw. Their size was about 1,600 cubic inches, and their diameter at the base about twelve inches. They seemed for the most part well filled, and the bees were hanging out in large clusters. Mr. Dathe remarked that the pasturage on the heaths this year was particularly plentiful. When I compare the deportment of these bees with that of my own, among which I can pass and repass daily without bee-cap or other protection, unattacked and unannoyed if I simply let them alone, I am no longer surprised that Dzierzon and other distinguished European apiarians declare that the Italian is much more docile than the German or common black bee. But I have had in my own apiary some black bees as docile as the Italians. I was, indeed, stung by bees of either race, but not more proportionally than the Rev. Mr. Langstroth was when I visited him two years ago, and we opened a large number of hives together. But to seat myself on the frames of a stocked hive of Italians, I should only venture to do after being protected in the rear by an ample application of Prof. Flanders' celebrated *bee-charm!*

After a long and agreeable interview with Mr. Dathe, I departed fully satisfied that he is one of the best practical apiarians. His little treatise on Italianizing common stocks, surpasses in brevity, thoroughness, and comprehensiveness, every work of the kind I have ever read. I have obtained his permission to publish a translation of the pamphlet, but whether it will be done, is among the uncertainties of the future.

From here I went to Bavaria to visit my parents, and thence to Bellinzona, Canton Tessin, the present residence of Professor Mona, where I arrived on the evening of the 11th of September. I may remark here that while crossing the St. Bernard, I made constant inquiry about bees, and found the last of the black race at Zising. A stage of four hours brought us to Splugen, where I was told there are no bees, the climate being too cold and rough for them. After another stage of four hours we reached the summit of the road across the Alps at this pass, and saw a peak elevated only about 300 feet higher, covered with perpetual snow. Though the conductor told us that this was one of the warmest days he had ever known in crossing, I found it cold enough to make an overcoat comfortable. Vegetation was sparse at the foot of the mountain and along the road-sides; and I am well convinced that no swarm of bees ever voluntarily passed across this mountain chain. After a brief detention on the highest point, we began to descend, and in five hours reached Bellinzona, situated about three miles from Lago Maggiore.

This morning Prof. Mona called on me at the hotel, to conduct me to his apiary, and in five minutes I had the gratification to see the Italian bees in their native home. Professor Mona's assistant, Mr. Uhle, a German from Hanover, immediately opened several populous hives, and showed me a beautiful yellow queen, and also a darker one with only some narrow yellow bands, but whose workers were as highly colored and as fully marked as those of the brighter queen. On my remarking that the darker queen would be pronounced impure in Germany, Prof. Mona and Mr. Uhle laughed and said the yellow queens were the exception, the darker ones having the normal hue; and assured me that their customers in Austria preferred and ordered the darker queens, alleging that they are hardier and more prolific. In Germany, however, the brighter queens are preferred, though he was himself of the impression that these are really not so hardy or long-lived as the darker. When I told Prof. Mona that some American bee-keepers contend that the genuine Italian queens are of a brownish color with the point of the abdomen black; and that others maintain that all the daughters must be duplicates of their mothers; he advised me to invite them to a personal examination of the Italian bees in their native land, the Canton Tessin and the adjoining Italian districts, and he was persuaded they would candidly admit their error. When I observed that some German apiarians alleged that the Italian bees were not altogether pure, even in their native land, but that there too black bees were occasionally found, he offered to carry me around among the neighboring farmers in a circuit of several leagues, and promised to give me a dozen queens if I succeeded in finding a single living black bee in all their stocks. I accepted the offer, rather from curiosity than from any expectation of success. Between nine o'clock in the morning and ten in the evening we visited a number of apiaries and examined the bees, without detecting the least variation in color or finding a single black bee. We found, indeed, a few apparently superannuated workers which seemed at first view to have only two yellow bands, but on closer inspection it was evident that the third had merely changed to a dark brown hue. In reply to a remark that my own Italian bees were much brighter-colored, with

the third yellow band of greater breadth, he said the circumstance was simply the effect of food and climate; that his bees also, when carried up the Alps during the summer, assumed a brighter color. I then told him that the workers produced by a queen obtained from the Rev. Mr. Langstroth, were not near so handsome as those from queens I subsequently reared from her brood; and that I ascribed their brighter color to the fine honey of the linden trees on which they subsisted almost exclusively. He expressed his entire concurrence in that view of the matter.

I again visited Prof. Mona on the 13th of September; saw how he prepared his queen nuclei for transportation; and examined the arrangement of the hives in which he sent off entire colonies. I must confess that I could not conceive of any mode better adapted to the purpose than that which he employs. I had the curiosity to inquire how many queens he sent off in the course of the year. He replied that this year the number would exceed two thousand. I was permitted to examine his account-book and found that he had filled nearly two hundred orders, prior to the first of April. The queens forwarded went to the several transalpine Cantons of Switzerland, to all the German States, to Hungary, France, and England. The orders are annually increasing in number. He showed me several letters just received, one of which ordered fourteen queens; another contained this remark: "My Italian stocks are very heavy, and all my common stocks, save two very populous ones, are light. Send me six more queens."

In the afternoon we visited the apiary of a Catholic priest, about a league from Bellinzona. We were kindly received, and when Professor Mona presented me as an American bee-keeper who desired to see his bees, he brought out a bottle of the finest wine I ever tasted to treat us. Learning in the course of conversation that I had about six hundred hives, he said that he had about two hundred, nearly all of which were distributed among the farmers of the neighborhood, who attended to them for half the profits. Here, too, I noticed at one of his hives several seemingly very old bees which were nearly black, but on close examination I could distinctly trace the faint remains of the originally yellow bands, which had now assumed a dark brown hue. The young bees had, without an exception, the three yellow bands. After this we visited another apiary, being the twelfth. I had already, on the first day, inquired of Mr. Uhle whether he was not occasionally stung by the Italian bees. He replied that this frequently happened, when he was opening queenless stocks, or colonies having only queen cells or an unimpregnated queen. He then requested me to notice and count the stings he might receive, as they affected him so little that he paid no attention to them, though never using a bee-cap. I must say that the Italian bees here seemed to me to be remarkably docile, though the heather and buckwheat were in full bloom—more docile indeed than my own in Wisconsin; and in the course of these two days, I was stung by them only once, though continually passing and repassing the fronts of their hives, and frequently taking up handfuls of bees.

Prof. Mona has principally movable comb-hives in his apiaries, but among the farmers every variety of ordinary hives are met with, both of straw and wood. The hives commonly used here seemed to me to be very small, in comparison with my own, containing only from 1,200 to 1,800 cubic inches; but the most of them were very heavy. Bee pasturage here is of long continuance, though not abundant at any one period.

The system of queen-raising adopted by Prof. Mona, appears to me to be efficacious indeed, but very slow. At the risk of being considered a boaster, I would say that if I had naught else to attend to I would rear as many queens in two months, as Prof. Mona and his assistant do in the entire summer.

Having now seen the bees of the Rev. Mr. Langstroth, at Oxford, Ohio, of Mr. Richard Colvin, in Baltimore, Md., of Mr. G. Dathe, at Eystrup, in Hanover, and those of Prof. Mona, and of the farmers in his neighborhood, and carefully compared the queens, I think I may claim to be qualified to form an opinion respecting Italian bees and queens. It struck me both last year and during the past summer that precisely those of my colonies which had particularly bright yellow workers, were on the average less productive in swarms and honey, than those with workers darker colored; and swarms from these yellow colonies, moreover, issued later than those from darker colonies and hybrid stocks. And I incline to coincide in opinion with Mr. Dathe, who, in the pamphlet already referred to, remarks that "very yellow queens are more delicate than those of a browner hue." Prof. Mona is of the same opinion. It seems to me, therefore, that those bee-keepers who desire to introduce the Italian race in their apiaries, not for the beauty of the bees, but for their greater productiveness, would do well to give preference to the darker hued, which are most esteemed in their native land. Though duly appreciating beauty, I should still greatly prefer a colony of pale yellow or dark colored bees that yielded me twenty pounds more of surplus honey, to a much handsomer but less productive yellow one. On the whole, I conceive we should more efficiently promote bee-culture, if instead of making it an object to rear beautiful yellow bees, we aimed at propagating from and multiplying the more industrious and most productive. For dairy purposes we certainly prefer raising calves from superior milch cows than from inferior milkers; and should not the same principle be applicable, with like advantage, in bee-culture? Would it not be practicable to improve the race of bees, whether black or yellow, by judicious selection in breeding? More than forty years ago, Ramdohr, a distinguished German apiarian, observed that colonies were frequently met with, which, with their progeny, greatly excelled others in industry and productiveness; and he advised that such colonies should be preserved and bred from.

In conclusion, I would state that Prof. Mona

has appointed me his sole agent in the United States, for the sale of Italian queens reared at his apiaries; and I hope to be able to effect arrangements in Bremen and New York, by which those who desire to obtain queens from the parent country of the *apis ligustica*, can be supplied directly from abroad on the most reasonable terms. I enclose an advertisement of Prof. Mona, and likewise a certificate from under his hand, that I have purchased from him one hundred Italian queen bees of undoubted purity. These I desire to have inserted. They will be followed in due time with a price current and further particulars.

Yours respectfully,

ADAM GRIMM.

[From the Canada Farmer.]

## Alsike Clover.

I had a small field of three and a half acres that I had summer-fallowed, and subsequently took a crop of fall wheat from it in the autumn of 1865. In the following spring I plowed it once and sowed to spring wheat, and seeded it down to alsike clover, putting on only five pounds of seed to the acre, harrowed in with the last harrowing. I should state that the field has had no manure since it was cleared, some eight or ten years ago. The clover germinated and came up well; and last fall I pastured it very lightly. In the spring of the present year, about the 20th of May, I sowed a barrel and a half of plaster on the field, and now I am cutting and securing the crop for seed. I have five good size wagon loads in the barn, and there are fifteen or sixteen more in the field. The average length of the stalks is about two and a half feet; but in some of the hollows it is as high as four and a half feet. Of course it was all down in one tangled mass, and it occupied eight long days for one man to mow it. It appears to be extremely prolific in seed. I think it would have been better to have pastured it till the first of June for a seed crop, as it would not then have grown so tall or been so badly laid.

In regard to its adaptation for bee pasturage, I find it excellent. During about four weeks it produced a multitude of blossoms, and the bees literally covered them from morning till night. Out of curiosity on the 24th of June, I drove out a common sized swarm of bees into a hive filled with empty comb, and having weighed them set them in one corner of the clover field. After the lapse of a week I weighed them again, and found that they had gained twenty-seven pounds. This additional weight was of course all honey, for there was no comb to build, nor could there have been any weight of brood in that short interval of time.

HONEY may be clarified by placing the vessel containing it in hot water, and continuing to skim as long as any scum rises.

BEES usually work more from eight o'clock in the morning till noon, than during the rest of the day.

For the American Bee Journal.

## Bee-Hives.

I have been much amused in reading the remarks on bee-hives in the JOURNAL; so, to amuse others, I return the compliment by sending the article below, taken from the *Rural New Yorker*. It was written in 1861, by "Abram Mudgett, of Great Valley, N. Y." I hope Mr. Mudgett "still lives," and that the Editor will mail him a copy of the JOURNAL containing the republication of the article, as I am not willing to deal with any one "behind his back."

I wish Mr. M. would let us hear from him through the JOURNAL, with regard to his birch-bark hive; and especially whether his combs still remain "white," and particularly whether he still continues in the "tame mink" business, since many in the West are anxious to get possession of something that will destroy the moth-worm. Now for the article:

### "A NO-PATENT HIVE.

Having seen models of different bee-hives in your valuable paper, I will introduce one for the benefit of bee-keepers, which is superior to any other in use. It is made of birch bark, and costs about five cents. My experience with this hive is five years. The comb remains white; it has grown no darker for the last three years; the bees winter on less honey, swarm earlier, and have one-third more wax. As the hive can be made to hold water, there is no hiding-place for the miller, although I have no millers about my apiary, as I keep tame minks, which exterminate them. I have kept from twenty to one hundred swarms for the last thirty years, and consider the birch hive, and the minks to catch the millers the *two most* important discoveries to bee-keepers that I have made in that time."

This birch-bark hive will doubtless please many readers of the JOURNAL, so far as *cheapness* is concerned. Just think of it, only "five cents" for a good substantial hive that keeps the combs "white," and is in short "superior to any other in use!"

M. M. BALDRIDGE,

ST. CHARLES, ILL.

[From the Iowa Homestead.]

## How I Wintered Bees in the Langstroth Hive.

I placed them side by side within two inches of the ground, drove stakes in the ground, and put in shingles and old boards, so as to leave a space of two or three inches all around the hive, except in front, (which was left open). This space I filled with straw; the honey box and honey-boards were taken off, and the caps filled with straw. My bees wintered in excellent condition, and consumed, I judge, a comparatively small amount of honey.

S. P. SNOW.

# THE AMERICAN BEE JOURNAL.

WASHINGTON, NOVEMBER, 1867.

☞ THE AMERICAN BEE JOURNAL is now published monthly, in the City of Washington, (D. C.,) at $2 per annum. All communications should be addressed to the Editor, at that place.


The revived discussion of the question whether the drones are, in any respect, affected by the fertilization of the queen bee producing them, has induced us to republish from Prof. Von Siebold's treatise on "*True Parthenogenesis in Bees and Butterflies*," that portion which relates to the reproduction of bees. The work was translated and published in England ten years ago, and a few copies have reached this country; but not having been reprinted here, the portion most interesting to them is not generally accessible to American apiarians. The article will occupy three or four pages in three consecutive numbers of the JOURNAL, and cannot fail to be instructive to inquirers.

Dzierzon, it will be seen, was the first to notice the phenomena which have also attracted the attention of apiarians in this country; and was, in consequence of his manner of treating them, charged with abandoning one of the chief points of his own theory. But Prof. Siebold conceives that the matter is susceptible of an explanation in no wise militating against that theory.

No *facts* have yet been adduced which we can regard as necessarily contravening the doctrine that fertilization is not needed for, and does not influence the production of drones; or as at all conflicting with the views expressed on page 175 of the second volume of the BEE JOURNAL.

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[For the American Bee Journal.]

"Woe be them Attic dapsterlings what writ
Them sassy 'pistles, with them plenty wit,
As dabbles 'bout them gums 'thout sides 'round it!
There's many a grins in lots o' scribblin' fiz,
Since 'talian flies gets piur, an bugs is riz."—Ignus Fatuus.

MR. EDITOR: Among the many futile attempts which have been made of late years, with a view to improve inventions which enlightened minds have hitherto considered perfect, none are more conspicuous and ludicrous than those pertaining to the culture of the honey bee. Extraordinary as has been the intelligential skill, wasted upon investigations innumerable as the stars in the azure vault above, in the fruitless attempt to discover how the human mind reasons, there is yet this to recommend the pursuit of the abstract sciences that they furnish fruitful themes of healthful, mental exercise, during hours of recreation, which could, in rainy weather, be not more usefully employed.

You may say Hobbes, "the man of a hundred heads," was *right* in demonstrating that there was no difference between right and wrong, yet you cannot say the same of the inventor of a bee hive who has never become proof against the sting of the bee. You may say Locke, Descartes, and Malebranche were *right*, had they proved to you that the human *mind* is impressed upon mule skin, yet you cannot say the same of the inventor of a bee hive whose *absence of mind* is stamped upon every part of his invention. And you may excuse David Hume even who *believed* that there was no *belief*, yet you cannot thus excuse the inventor of a bee hive who *believes* that everybody *believes* there is no *belief* as perfect as his own.

Simplicity being the first predicate of perfection, it follows that complicated bee hives are a nuisance. Porosity, lightness, and warmth being the three prerequisites of a perfect material for a bee hive, it follows that in the absence of corktrunks we have to rely on straw and porous wood; and now since the durability of these has become objectionable on account of their negative qualities in these respects, perforated sheet iron, tin, and zinc, and cast iron, with the addition, perhaps, of steel and platina, bid fair to take the lead as articles, per excellence, in the construction of all *perfect hives*, excepting the only good for nothing, old fashioned, ugly-looking, ill-shaped, bee-killing, honey-producing "cracked old kittle," which cost Prof. W. A. Flanders & Co., and "smaller 'taties," such an immense amount of lungs, cash, and literary efforts, to reclaim from the inevitable doom of oblivion.

But, Mr. Editor, my purpose was not to be seduced by a subject so fascinating, for I have got a bee hive of light, durable, airy material and construction, which I have been so fortunate as to invent, through the instrumentality of Prof. Flanders' *invaluable* "Bee Charm." (See BEE JOURNAL volume 2d, pp. 186, 206.) There is no patent obtainable upon it, and therefore, my dear Mr. BEE JOURNAL, I would like to secure your assistance in the perpetuation of another impeachable good upon all who are in favor of getting the cheapest, bestest, and most handiest bee hive in these United States and all America besides. I've got a whole book of Greek and Latin quotations all ready for any emergency, and if I should not succeed in my laudable enterprise, I will send the book to any editor, of good standing, with one of my immovable, immortal, and immaculate bee hives, free gratis for nothing at all, except a puff or two in his county paper.

Meanwhile, I shall remain, as before, respectfully, your super-prolix apiloquax,

Prof. APIASTER ALSATIUS, A. M.,
Corresponding Secretary of Coon Island
Golden Apiary, 12½ miles from shore.

☞ There's lots of folks what think them there tin skips of mine more better as any they was used to.

[For the American Bee Journal.]

## Experience of a Novice.

### No. 8.

MY ONE EXCEPTION TO ARTIFICIAL SWARMING.

Those of my readers who have followed me thus far may recollect that in my first attempts at queen-raising I finally removed my Italian queen from her hive (which hive was the Langstroth I had so carefully made from the book); and that I succeeded in getting quite a number of queen-cells.

Well, as the Italians had built queen-cells so readily, and, most of all, were so much quieter and easier to handle than my black bees, I concluded to keep them at the business. Accordingly I used all the cells and gave them fresh brood to raise more. This time I left them one cell; but, strange to say, they clung to the young queen, and killed her almost as soon as hatched.

Well, "my beauties," thinks I, I will have to try once more; and I gave them a whole frame of brood to encourage them to keep up their spirits for ten days more. In eight days and twenty hours, according to a memorandum kept at the time, I opened the hives to count my queen-cells. I found them all torn open, and on looking further found the largest queen I have ever seen—"a young giantess," as some one has expressed it. The bees were then working on the blossoms of the basswood—a coincidence with the case of the great number of queen-cells raised on one occasion by Mr. Grimm, as stated in the BEE JOURNAL, Vol. 2, page 170.

In a few days more I found that she had almost filled the combs with eggs, which hatched in due time. The bees proved to be hybrids. As our fall last year afforded no pasturage, it took nearly all of their honey to raise the brood; and in November they were nearly destitute. I treated them to twenty pounds of sugar syrup with the rest of my stock, and did not see my "tall" queen again till the warm weather in February. Then I found her filling her combs with eggs at a great rate, and some days after found quite a quantity of larvæ and immature brood in front of the hive. I let it pass, however, supposing some of the brood had got chilled and were carried out by the bees.

Toward the last of March, on opening the hive I could not find a particle of brood, and scarcely any honey, and the bees much dwindled down. I had no Langstroth frame containing any honey; but near by stood and American hive that had died out from being too old, as mentioned before. I now saw the inconvenience of having two kinds in an apiary. The American was nearly full, but I could not exchange the frames; so I decided on what I then considered a bold experiment, as it was pretty cold weather at the time. I put the American hive in place of theirs, and brushed them off, "giantess" and all, nearly starved and stupid with cold, into the cold hive, picked up those that fell on the ground, tumbled them in and left them to do the best they could. But towards evening, feeling some remorse about the rough manner in which I had treated them, I ventured to look in upon them, and found that they had so far recovered as to remember that they were *hybrids*, and objected to any further assistance. Still I managed to get a peep at "giantess" and the preparations they had made for housekeeping with the benefit of plentiful supplies.

After that, as they had become quite weak, I decided to let them build up as fast as they liked; and when I began to take frames from the rest to raise queens, my wife bade me let the one with the large queen alone and see how strong they would get, as she had doubts of "my policy" in artificial swarming, &c.

Towards June they seemed pretty strong, but had not got their combs quite down, and had not clustered out any; yet, for fear they might swarm, I did remove a comb from the centre.

Two days after this, it being necessary for my wife to be away, I requested a neighbor's wife to watch and inform me at the store, which was not far off, if my bees should swarm. Sure enough, about ten o'clock, she came in out of breath to tell me they had swarmed. Contrary to all known rule and regulation, they had not waited to cluster or anything of the kind, but poured out of the hive "pell-mell," as if Old Nick himself was after them; hurried up into the air, and off in a northeasterly direction—the largest swarm that had ever been seen, as all agreed.

Of course it was "giantess" on the rampage. I had been thinking of clipping her wings only that very morning, and now she was gone. In despair I seized my hat and set off in the direction indicated; but only succeeded in learning that they had been seen to pass over a certain house, perhaps twenty rods, from my apiary. Resolving that I would get them yet, if within the bounds of possibility, I returned home to make examinations.

I found the hive almost entirely destitute of bees, but every cell filled with brood and honey; even the frame I had given them, partly filled with empty comb, was full nearly to the bottom, and the outside frames solid with sealed honey. But, strange to tell, not a queen-cell in any stage was to be seen, and scarcely bees enough to build one, as it then seemed.

Perhaps it may be as well now to follow the fortunes of this hive to the end, as the novelists say, if my readers will allow the digression. I have always had much trouble with my young queens, just before or about the time they began to lay. One in particular, I used to expect to find clustered with her quart of bees on some neighboring tree in spite of brood, larvæ, or eggs, almost every evening when I came home. Well, just the Sunday morning previous to the event narrated, as I was enjoying the cool air in the garden among my bees, with rather a disregard to habiliments, that is, without hat, coat, or shoes, I was surprised to hear faintly the peculiar hum of swarming bees, and just caught sight of my would-be truant nucleus going over the fence. As its queen was a fine one, I started in pursuit, seizing my bee-hat and a queen cage on the way. Over the fences and

gardens we went, across several streets, presenting rather a queer sight to the good people on their way to church, and only stopped at a friend's apiary, some twenty rods or more off (the very one before mentioned), who was just hiving a swarm of black bees, with which my miniature swarm seemed desirous of forming a partnership. (*Query*—Could my bees have heard the swarm coming out at that distance, or was it merely accidental?) I stationed myself at the entrance of the new hive, and as her majesty made her appearance, I prevailed on her to enter the queen-cage, and carried her back home—a part of her subjects being loyal enough to follow. On the way home, I bethought me of an expedient to make her stay—strange I had not thought of it before—"clipping her wings," namely. I had tried the seven-sixteenths of an inch arrangement, but where the bees passed through, the queen had managed to go also, as had been the case that very morning. I accordingly got the scissors and clipped one wing; and to make assurance doubly sure, thought I would clip the other wing also. This seemed to cut rather hard, but I did make it come; when, behold, I had cut off a foreleg too, which she had thrust between the blades in the insane attempt to save her wing. She was put back, and staid at home at least until the opening of our story. When I found that the deserted hive had no queen-cell, I went immediately for the clipped queen; but she too had caught the fever and gone. If she had no wings she had feet, at least a part of them, and as she could do no better, had gone *on foot*. By watching the few remaining bees that remained around the small hive or box, I actually saw them follow on her track, when I found her nearly half way across the garden, surrounded by her remaining subjects. From the manner in which they followed her trail, I think that she must have left a peculiar scent along the ground she had traversed.

She was caged again and given to the swarm, or rather the hive where the swarm was; and as the few bees left did not seem hostile at all, she was released after a short time, and I supposed all was right. But after a week or more, finding them queenless, with a large number of queen-cells started, I concluded that either they had killed her, thinking they could raise a better looking one, or that she had taken another fancy to a pedestrian exploration. My wife suggested that if I had not clipped her wings on *Sunday*, she might have proved more profitable.

I am really afraid that that truant swarm will have to stay away until another month, or something valuable may be crowded out of the BEE JOURNAL, to give room for the long article from NOVICE.

☞In the article in the JOURNAL for last month, page 65, second column, in the 18th line from the bottom, the word "improve" should be "impure."

## To unite Bees in the Fall.

I alarm the bees in both hives which I wish to unite, then leave them a few movements to fill themselves with honey, I then put one of them over an empty hive, (my hives have movable bottoms), take each frame out, and shake or brush the bees into the hive below. When all are out, set the other in its place, and proceed in the same way. The bees all brushed together thus in an empty hive, are too much frightened to quarrel. I then arrange all my frames containing honey in one hive, and set it over the one in which the bees are. They all go up rapidy and take possession of the frames like the colony. One of the queens will, of course, be killed; and hence, if you have any choice between them, search for the one you least care for, and destroy her.

Every empty comb should be saved. Indeed no piece of good worker-comb should ever be melted for wax. It is worth five dollars a pound in honey boxes, or fastened into the frames for the use of the bees.—Mrs. E. S. TUPPER, *in Iowa Homestead.*

[For the American Bee Journal.]

## A Profitable Apiary.

Silas Way, of this county, is one of our best bee-keepers. He has now two hundred and thirty hives of bees—all natives. He wintered over one hundred and twenty-five colonies, and from them had nearly two hundred natural swarms. Many were consolidated, and the whole number of swarms were put into one hundred and five hives. His crop of honey in small boxes amounts to nearly two tons, which he is selling at thirty cents per pound. He has one hundred and twenty-five colonies in the the improved Langstroth hive; the remainder in box hives. The shallow form of this hive is his preference, as he claims that he can get more surplus honey from low broad hives than from tall ones. From eighteen Langstroth hives that did not swarm, he has secured 1,080 pounds box honey—sixty pounds, worth eighteen dollars, per hive. Mr. Way loses no bees in winter. They are wintered in a dry dark cellar, directly under the living room of his house. His greatest difficulty has been to find room enough in the tops of his hives for the accommodation of the bees that store surplus honey. He has a plan now that he thinks will obviate this difficulty. The inside depth of the comb-frame that he uses is 9½ inches; but he purposes to make one hundred more this winter, with frames only 8½ inches in the clear. This change will give him more room for boxes, and with two sets he thinks he may be able to give his bees all the room they can use to advantage. He thinks he can get by this arrangement, coupled with good management, 100 pounds surplus honey from any good hive of bees that is kept from swarming, provided the season is an average one for this latitude. On his box hives he uses two honey-boxes; but on the Langstroth hive he can use three of the same size. He claims that the three boxes on frame hives are filled with honey as soon as the two on box hives. Mr. Way does not *work the frames* at all; in fact, pays no more attention to frame hives than to box hives. Still, he finds

it for his interest to use them. His hives are made in the simplest and cheapest way that he can devise, and do not cost him to exceed two dollars per hive complete, with one set of boxes They are all planed and painted, and present a fine appearance.

RECAPITULATION.

| APIARY. | DR. |
|---|---|
| To 125 hives of bees, at $8 each, including hives. | $1,000 |
| 10 per cent. interest on capital. | 100 |
| | $1,100 |

| APIARY. | CR. |
|---|---|
| By 230 hives of bees, including hives, at $8 each. | $1,840 |
| 4,000 pounds of honey @ 30 cents. | 1,200 |
| | $3,040 |
| PROFITS, | $1,940. |

The exact yield of surplus honey is not known, but the above amount is not far out of the way. If materially wrong, I hope he will correct the mistake. M. M. BALDRIDGE.

ST. CHARLES, ILL.

[For the American Bee Journal.]

## Melilot Clover—No. 2.

Melilot clover came into blossom this year early in July. For a few days in August there were no blossoms—about ten days in all. This is the first season that the plant failed, in fair weather, to give a supply of honey for a single day after it began to bloom. About the middle of August a fresh supply of blossoms made their appearance. Since that time the plant has been constantly in full bloom; and to-day, (Oct. 15), the supply of blossoms is as large as at any other time this season. In fact, the plant is literally white with blossoms, and is visited as freely by the bees as one could wish. The past three days have been warm and pleasant, being fine weather for the bees to work. There is not a doubt but the plant will remain in bloom till winter sets in, although we are having a serious drouth—the worst we have had for many years. Pastures are nearly ruined for present feeding; and many farmers, and others, have been obliged to fodder their stock. Many kinds of weeds are "dried up," that usually are green and vigorous at this season of the year. We are therefore much encouraged with regard to the value of Melilot clover for bee pasturage in seasons of *drouth*. We have also had some frosts, but, as stated in my former article, they do not injure the blossoms of this clover. In this respect Melilot clover is as hardy as what are known as the "frost flowers" of the prairies.

I have now some *fresh* seed that I will mail, in small parcels, to all applicants on receipt of a few stamps for postage and putting up. The seed may be sown any month of the year.

M. M. BALDRIDGE.

ST. CHARLES, ILL.

[For the American Bee Journal ]

## Northwestern Bee-Keepers' Association.

MEETING HELD ON THE IOWA STATE FAIR GROUNDS.

Notice was given out during the Iowa State Fair, that there would be a meeting of bee-keepers on Friday afternoon, October 4th, and those interested in bee-culture were invited to be present.

Much interest was manifested in the movement, and about 150 bee-keepers were present at the meeting. An organization was perfected, entitled "THE NORTHWESTERN BEE-KEEPERS' ASSOCIATION."

The following were elected as officers of the association:

R. R. MURPHY, Fulton, Ills., President,
DAVID HESS, Lyons, Iowa, Vice President,
M. M. BALDRIDGE, St. Charles, Ills., Sect'y,
H. A. RODGERS, Treasurer.

COMMITTEE ON CONSTITUTION.

J. Bullard, Evansville, Wis., M. M. Baldridge, and James M. Marvin, of St. Charles, Illinois.

The main object of the meeting was to organize. There were many questions asked and answered with regard to bees; but no notes were taken of these proceedings. The AMERICAN BEE JOURNAL was highly endorsed by its readers present, and all bee-keepers were advised to become its subscribers and supporters. A request was made that these notes be sent to the AMERICAN BEE JOURNAL and other periodicals friendly to the association, for publication.

The meeting then adjourned, subject to the call of the Secretary. Should the State Fair be held on the same grounds the ensuing year, there will doubtless be another meeting at the time of the Fair. Notice will be given in the BEE JOURNAL and other papers, when the matter is decided. Meantime the Secretary invites correspondence, with suggestions as to the time and place of holding the next meeting. It may be advisable to have a session next spring. The Secretary would be pleased to receive the names and post office address of those who desire to become members of this association.

M. M. BALDRIDGE,
Secretary.

ST. CHARLES, ILLS.

The queen bee is the soul of the colony. No bee-keeper should undertake to winter a queenless colony. It is inevitably doomed to destruction, and all efforts to carry it through the winter in that condition are bootless—involving a waste of honey, and a misapplication of time and labor. Unite the bees in the fall with some other colony, and fumigate the combs with brimstone, to preserve them for the use of early swarms next spring.

☞ SEND us names of bee-keepers with Post Office address.